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CLINICAL REMARKS ON A CASE OF EXTRACTION OF CATARACT.*

Delivered Jan. 3d, 5th, 6th, 7th, 9th and 20th, 1863, by Prof. A. VON GRAEFE.

[Translated from the Klinische Monatsblätter for 1863, p. 141, for the Boston Medical and Surgical Journal, by HASKET DERBY, M.D.]

MRS. D., 64 years old, has a hard, ripe cataract in the right eye, an unripe one in the left. The external appearance of the eyes indicates no complication. The cataract of the left being unripe, fingers can be counted in 2′, while the perceptive power of the right eye has been measured by means of our "graduated diaphragm." If we use an entirely darkened chamber, placing the diaphragm 8″ (the distance which for the sake of uniformity we always employ) from the eye to be examined, and reduce the intensity from 100 to 1, the patient gives entirely correct answers till 4 is reached, but at 2 begins to be uncertain. This is exactly what we should expect when the cataract is ripe and saturated, as is here the case. If the diaphragm be set at 25 and moved from side to side, the patient follows it with the eye as accurately as could be expected, considering the diffusion of light. The question as to whether any considerable near-sightedness had previously existed, a question which—in view of the frequent combination of cataract with *sclerectasia posterior*—should never be omitted, was also answered in the negative. In a word, we have before us a so-called *cataracta simplex*.

* From a letter of Von Graefe to the Editors:—

"The case which forms the subject of this lecture is by no means an exceptional one. I am, however, of opinion that a description of those irregular complications that may occur during the healing process after flap-extraction, as well as an allusion to various points that are now, in this connection, receiving special attention, may be of use to the practitioner; and do not hesitate, therefore, to offer some such material to the 'Klinische Monatsblätter.' The most assiduous observation and the most comprehensive experience avail of course only up to a certain point in answering the question why flap extraction is sometimes followed by unfavorable results, just as the problem in general surgery, as to why wounds, that show every disposition to unite by first intention, sometimes suppurate, is capable of an only partial solution. Still we may attempt to approximate to it by carefully noting and comparing the different circumstances influencing the patients, the eyes, the operation and the after-treatment in cases resulting favorably and unfavorably. And although I reserve for a special volume the results of my own experience in this connection, embracing at present 1500 cases, I am still disposed to take a few cases of flap-extraction to illustrate some clinical remarks bearing on this subject."

I say intentionally *so called;* for strictly speaking it may be said that all eyes, in which cataract is spontaneously developed, may be found to have undergone more or less change in other respects, particularly in the hyaloid membranes and vascular system: in fact, these changes may well come to be looked upon as the pathological point of departure of the cataract; meanwhile, however, the very great differences in respect to recovery, which at present fill us with astonishment and make us almost lose confidence in ourselves—especially when we compare them with the results of traumatic affections of otherwise healthy eyes—these differences, I say, may be explained by a closer study of the nature of the anomalies which are at the bottom of the formation of the cataract. Such theorizing, however, carries us into an obscure future; it suits our present purpose to limit *cataracta simplex* to those cases where we are unable to discover any affection of the deeper structures of the eye, and no break exists in the nervous apparatus.

Shall this patient be operated on at present? There is certainly no valid reason for delay; in fact, we hold it prudent to do the operation of flap-extraction on only *one* eye at a time,* and it is only in exceptional cases and under extraordinary circumstances that we depart from this rule. In one eye the cataract is ripe. Did the patient enjoy tolerable vision with the other eye, and could she gain her living, we might advise a postponement of the operation; for three months, however, she has not been able to go about alone, which fact makes the recovery of sight a matter of prime necessity under any circumstances.

Let us now come to an understanding with regard to the *prognosis of a flap-extraction* in the present case. And first, as to the general state of the system. The patient, it is true, is only 64 years old, but prematurely marasmic. This is sufficiently shown in the deep wrinkles, the atrophy of the skin all over the body, particularly on the neck and backs of the hands, the scanty and thinly sown gray hair, the somewhat stooping form and a weight of 88 pounds to a height of 5 feet 3 inches. In my estimation, marasmus should exert a most unfavorable influence on the prognosis of flap-extraction, that which is premature more than that which legitimately belongs to the time of life.† But we find, moreover, in the eyes themselves circumstances which contribute to depress our hopes, namely, the deeply sunken position of the balls, the small corneal diameter, which we have just found to hardly measure $4\frac{3}{4}'''$, and a tremulousness in the muscles when the eyes are turned far in or out. A sunken position

[* In view of the difference of opinion on this important point, which has prevailed and to some extent still exists in this country, the translator would call attention to the accompanying passage from the excellent work of Wecker, now in process of publication:—

"The elementary principles of prudence indicate sufficiently the impropriety of performing the operation for cataract on more than one eye at a time. In the first place, the consequences of the first operation and the results which it furnishes are, for the surgeon, the source of valuable information with regard to the line of conduct he should pursue in the second. In the second place, no conscientious operator would consent to expose his patient to the risk of losing, at one cast, all hope of recovering sight, a thing which might happen after a double extraction, followed by accidents entirely independent of the operation itself, and determined by the imprudence of the patient or of the persons surrounding him."—*Etudes Ophthalmologiques*, tome ii. p. 255.]

† The opinion of some colleagues that extreme age does not exert a particularly unfavorable influence on extraction is most emphatically contradicted by my tables, which show after the 65th, and particularly after the 70th year, a considerable falling off in the percentage of recoveries.

of the balls has been particularly dwelt upon as an unfavorable circumstance, in so far as it interferes with the mechanical execution of the operation. Viewed in this aspect, I do not attach much importance to it, for when extraction is done downwards a moderate amount of practice enables us to overcome the difficulties in question. But an abnormally deep position of the eyes, depending, as it does in general marasmus, on the disappearance incidental to age of the fatty tissue of the orbit, is, it is somewhat probable, an unfavorable sign as regards recovery; in fact, it is very probably so when, coupled with it, is the second sign to which we have referred, the senile diminution in diameter of the cornea. Where such a combination exists we are almost sure to have collapse of the cornea after the operation; that peculiar form of collapse in which the cornea not only falls in, lies loose and in folds, but also exhibits a concentric shrivelling, the whole indicating that the corneal tissue is capable of but little resistance. Experience shows more and more that the loss of nutrition which is brought about by the flap-section is fraught with peculiar danger to such a cornea, and is followed not infrequently by total or partial suppuration. The same thing happens in the case of these thin and but slightly elastic corneæ, as in a plastic operation where the skin is thin, atrophic, and devoid of panniculus; a loss of nutrition that, under more favorable circumstances, would be perfectly supportable, giving rise to fears of suppurative destruction or entire necrosis, an unusual amount of contraction being observable in the skin as soon as separated from its original insertion. Finally, the third sign, viz., the nystagmic trembling of the muscles when made to exert their utmost contractile power, goes to show a senile atrophy of the muscular apparatus and to confirm our fears that the wound may not heal well. *It is thus made evident that the danger of a diffuse (necrotic) as well as of a defined (flap) suppurative process is extraordinarily great in the present case.*

The tendency, too, to *iritis* is decidedly greater in marasmic eyes (increasing, as it does, in proportion to the age). The hardness of the cataract tends to render this more probable, greater violence being done as it passes through the pupil. Still, I should not say that the affair promised particularly ill in this respect; for one of the cataracts is entirely ripe, from which we infer an entire and easy separation of it from the capsule, and have neither to apprehend difficulty or delay in the removal of the cortical masses, nor that any will remain behind—a fertile cause of iritis. Moreover, atropine brings about a complete dilatation of the pupil, a small border of iris alone remaining, and it continues well dilated for four or five days, a thing by no means universal with old people, and which indicates relatively less tendency to iritis. In order not to be misunderstood, I should here add that I consider the iritis following extraction as capable of classification under two heads—*transplanted* and *genuine*. The transplanted follows upon or becomes developed with the suppuration of

the wound, the latter either leading to a cell-growth, on the posterior surface of the cornea, which pushes into the pupil, or else the suppuration seizes upon the portion of the iris in the immediate vicinity of the wound, and thence invades the remainder of the iris and the ciliary region. The genuine iritis, which seldom comes on before the third day, and generally makes its appearance between the fourth and tenth, has no direct effect on the healing of the wound, but when of early occurrence may influence it unfavorably, though indirectly. This form alone was, of course, that to which we had reference, the transplanted variety having its own exciting cause.

Finally, it should be stated that the patient suffers from an old stoppage of the lachrymal passage and moderate epiphora, a circumstance which is shown from experience to somewhat diminish the chances in favor of the healing of the wound.

Taking all these circumstances into account, the general prognosis of flap-extraction must be here essentially modified. According to my reckoning, of a hundred cases of flap extraction 65 result favorably, by which I mean the gaining an acuteness of vision of at least $\frac{1}{4}$; if more than 75 years of age, at least $\frac{1}{6}$. In 15 of the remaining 35 a favorable result is attained by a subsequent operation, consisting either in an operation for secondary cataract, or in an iridectomy with an operation for secondary cataract; of the 20 that now remain about a third get at least vision enough to go about alone (acuteness of vision $\frac{1}{50}$ to $\frac{1}{30}$), a second third gain still less, and from 6 to 8 per cent. of all eyes operated on remain or become entirely blind; that is, deprived of all ability to distinguish objects (whether they have quantitative perception of light or no). This is the final exhibit, when I take into account all the cases of *cataracta simplex** where an operation seems indicated.

Under other circumstances much more flattering statistics could be shown; my own, for instance, would have been twice as good if I had only included the operations on the occupants of the private rooms of my infirmary, and omitted the poor patients, the majority of whom were operated on under very unfavorable circumstances. Far more favorable results, too, might be furnished by those practitioners who either employ another kind of operation in unfavorable cases, or decline such cases altogether, than by those who perform the operation of flap-extraction in all those cases in which an operation is not contra-indicated by the tenets of our science. Finally, I am convinced by my own experience that the recovery after extraction, like that after all surgical operations, is influenced by differences of climate. I therefore lay only a relative stress on the above statistics, and communicate them solely in order to furnish an average scale to the less experienced practitioner, who may wish to make a truthful statement to his patient of the chances of the undertaking; for the sake, moreover, of assigning to the present case its individual place on the general scale of prognosis.

As may be readily seen, the prognosis of extraction may be infi-

* The more marked cases of myopia were formerly excluded from the list; latterly, however, have been reckoned in, provided no amblyopic complication could be discovered before the operation. This seemed allowable because, contrary to anticipations, experience has shown the healing process to be in no wise unfavorably affected by the staphyloma posticum (provided the vitreous is not considerably changed).

nitely better in a single case than in the long run, and *vice versa.* Imagine a patient in the vicinity of fifty years of age, perfectly healthy, of an equable temperament, submissive, hopeful as regards his future, with a large cornea of 5¼‴, a cataract ripe for the last quarter or half year, and with soft cortical substance—the chance of success would here be exceedingly good, infinitely better than the given average. An opposite condition of things inclines the scale in the contrary direction, and thus is it in fact in the case before us. I should here hardly estimate the chances of immediate and entire success as more than even, while the chances of an ultimate recovery, to be obtained by a secondary operation, are as 2 to 1.

Is, then, for this reason, another plan of operating to be selected? The hardness of the cataract forbids the thought of discission, an operation, indeed, seldom advisable in advanced age; nor are we likely to choose linear extraction, with or without iridectomy, an operation which, in cataracts as consistent as the present, does too much violence, leaves cortical substance behind, and is followed by chronic iritis, which generally invades the ciliary region. Our sole choice, therefore, lies between flap-extraction and reclination. The chance, on the whole, after reclination is very unfavorable compared with that after flap-extraction. If the average success following the former is to be reckoned as at least 80 per cent., that belonging to the latter is at the outside 50 per cent. Still, a fair field for reclination might be found in those cases where the individual circumstances cause the chances of extraction to fall considerably below the average. It is, however, in this connection to be regretted that most of the physical objections which, in a given case, apply to flap-extraction, may be urged with equal or greater cogency against reclination. Thus it is, for example, with certain complications which have been actually brought forward as contra-indications to flap-extraction, for example chronic choroiditis with a fluid condition of the vitreous. Their existence, of course, interposes a serious obstacle in the way of extraction, but a still greater one in that of reclination. Supposing, then, an operation to be advisable, they are not to be considered as contra-indications to the former, but simply as unfavorably modifying the prognosis. The same is true, though perhaps not so emphatically, of marasmic eyes like the present, unable as they are to long endure the irritation set up by the displaced lens, the tendency being to the development of chronic cyclitis or deep-seated inflammations, attended by increased secretion and ending in excavation of the optic nerve, the whole contributing to establish a prognosis for reclination much below the average. In my opinion, reclination is to be regarded as an incontestably proper though exceptional procedure in those cases where the general state of the system renders the healing of the wound doubtful, and where the danger of choroiditis or of inflammation with hypersecretion does not increase in the same ratio. After all, a too exclusive predilection should not be allowed to turn

the scale in balancing chances. Signal as seems in the light of to-day to be the blunder of submitting to the insidious operation of reclination an eye which offers an average or even better prospect of success from extraction, there yet remains scope for the exercise of some choice or preference on the part of the operator in cases like the present, where extraction offers a decidedly diminished chance. And although in the case of our patient extraction seems on the whole more advisable than reclination, we should readily comprehend and in no wise find fault with the course of any colleague who employed by preference the needle.

Flap-extraction having once been determined on, we have to decide whether to *precede it by an iridectomy*, or perhaps combine the two operations. What advantage is to be gained by combining an iridectomy with a flap-extraction? Does it ward off the danger of *diffuse suppuration of the cornea*, a process occurring as a general thing between twelve and twenty-four hours after the operation, and characterized by profuse secretion, appearances of swelling and the rapid formation of a purulent infiltration encircling the entire cornea, and premonitory of necrosis of this structure? Not in the least. We have seen this very thing occurring in the same manner and running the same course in cases of extraction where the precaution of making an artificial pupil had been adopted. Only in proportion as the presence of the coloboma renders the mechanical execution of the operation in itself more easy, can it be said that an indirect influence has been exerted on the occurrence of the above process.

Does the presence of the coloboma afford any protection against *defined suppuration*, the symptoms of which generally make their appearance somewhat later (eighteen to thirty-six hours), are otherwise externally similar to those of diffuse suppuration, except that the secretion is less abundant, and after having been poured out for the first time somewhat diminishes, in which, however, the suppurative process confines itself to the vicinity of the wound or the corneal flap, and at the most exhibits a tendency to send a ring-shaped infiltration into the uncut portion of the cornea? This query, too, must be answered in the negative. Since the time that I have combined iridectomy with extraction to meet various indications, I have not noticed any influence of the procedure (making again the same allowance as above for its effect on the act of the operation) on the *occurrence* of defined suppuration of the wound, but have, however—and this is a very important point—on its *course*. The principal danger of defined suppuration, always supposing it does not ultimately become diffuse, lies not so much in the destruction of the cornea, as in the iritis transplanted from the wound (see above). The masses of pus make their way into the anterior chamber, the iris becomes the seat of suppurative swelling, and this transplanted suppurative iritis exhibits a peculiar tendency to invade the ciliary region, and thus lead to irrecoverable loss of sight from ciliary exu-

dations and atrophy of the bulb. The existence of a coloboma of the iris does not, it is true, remove the possibility of such an invasion, but is undeniably efficacious in hindering its progress. The process attains a diminished height, union of the edge of the pupil with the capsular cavity takes place more quietly and frequently to a less extent, and purulent cyclitis fails to make its appearance in many cases, where, but for the coloboma, it might have been expected.

Does the iridectomy afford any protection against *general iritis*, the development of which is in most cases owing to contusion of the iris during the operation, or to the leaving behind of cortical remains? To a certain extent, we need not hesitate to answer this in the affirmative. The protection furnished is, of course, by no means absolute; genuine iritis, however, is less frequently observed where iridectomy has been done in advance, and when it does occur is, as a rule, less severe. It is plain that the amount of contusion inflicted during the operation is less when a coloboma of the iris exists opposite the apex of the flap. And, where the amount of contusion is the same, an iris in which a coloboma has been made exhibits a lessened tendency to inflammation, and inflammation of it is less dangerous; on all sides, therefore, are favorable circumstances which give us an *a priori* ground for foreseeing the result which experience renders secure. It is, moreover, indubitable that iridectomy interposes a serious though not invincible obstacle in the way of a *prolapse of the iris*, and appreciably lessens the dangers of such a prolapse.

We have alluded in this connection only to the more common events that interfere with the healing process after extraction. There are others, more exceptional, however, in their character. To these belongs a disease which is clearly to be distinguished from genuine iritis—*iridophacitis* (observed six times in 1200 cases)—a state in which, while the inflammatory symptoms gradually increase, the whole capsular cavity becomes transformed—through hypertrophy of the intracapsular cells—into a bag of pus, while the iris at first plays but a subordinate part in the inflammation, and the ciliary region becomes only subsequently affected. In this category is, moreover, to be classed the affection to which I have given the name of *phagedenic wound-pustule*, and which may bring about a disastrous result as late as the third or even the fourth week; its occurrence, however, being fortunately even less frequent (four cases observed out of 1500). These processes are not, be it incidentally remarked, particularly affected by iridectomy. It accidentally happened that the most of our scanty observations were made on eyes on which a preparatory iridectomy had been done. Direct purulent infiltration of the vitreous, a thing that may develope itself after a loss of this humor or intra-ocular hæmorrhage, should also be reckoned in, were we making a careful statement of the things that might take place after an extraction, and not simply sketching their relation to iridectomy.

On a general review of the foregoing, it appears that iridectomy offers no protection against the occurrence of diffuse and partial suppuration, while on the other hand it does, in the case of the latter, go to ensure a more favorable course of things, and to a certain extent prevents the occurrence of iritis and prolapse of the iris. We may thence deduce the general principle that a portion of the dangers attendant on extraction are obviated by iridectomy, and that

this operation is therefore to be employed where such dangers are imminent. As regards the particular indications, I stated several years ago (*Archive of Ophthalmology*, vol. ii., part 2, pp. 247–248, 1854) that iridectomy should always be done when the performance of the operation was attended with any difficulty; for instance, when, owing to the small size of the flap, to too small an opening in the capsule, or to too much adherence of the cortical substance, the lens did not slip easily out, but advanced with evident difficulty. Then I laid stress on iridectomy in cases where prolapse of the iris seemed probable, owing to unsatisfactory juxtaposition of the edges of the wound, to a tendency of the pupil to prolong itself in the direction of the wound. I do not, moreover, fail to perform iridectomy where entirely hard cataract exists in connection with a small pupil, difficult of dilatation; where cortical masses not yet completely ripe are with difficulty removed or have to be left behind, or where the former is true of a case of ripe cataract owing to the consistency and adherence of the cortical masses. Iridectomy, too, is to be advised in any case where the chance of a good union of the wound is small, because it may possibly contribute to a favorable result in the event of defined suppuration.

Is iridectomy always to be combined with extraction? Were the dangers of the operation actually diminished thereby, and were the procedure itself unaccompanied by any drawbacks, it would seem as though the question must be answered in the affirmative. Let candor, however, be scrupulously observed. The thing is by no means without its drawbacks. In the case of very restless patients its simultaneous performance meets with some obstacles and is attended with some anxiety; on neither of which points, however, am I inclined to lay as much stress as does my good friend Mooren, who has recently in a very meritorious manner drawn public attention to the combined procedure. An iridectomy done some time beforehand has the one decided disadvantage of subjecting the patients to a double operation, prolonging their stay in the hospital, and of sometimes, through delay, destroying their moral courage; indeed, if the principle of a long interval be defended, it is often incompatible with external circumstances. More especially, however, is the fact undeniable that the vision of patients having a coloboma downwards and no accommodation, labors under some disadvantages—it not being necessary to regard cosmetic considerations with old people—when contrasted with cases where the pupil is central. These disadvantages apply less to the amount than to the distinctness of vision, and obtain particularly in instances where little irregularities of curvature or cloudy opacities of the cornea result in the vicinity of the wound after extraction, as not infrequently happens. These disadvantages are, to be sure, of minor consequence; inasmuch, however, as iridectomy directly benefits only the minority of operative cases, and it is exceedingly probable that its performance under favorable circumstances is superfluous, I am unable, after weighing the pros and cons, to pronounce in favor of its general adoption in cases of extraction. It gives me pleasure, however, to see it done by others,* because an evident advantage accrues to science from its general application and a conscientious determination of results.

* The proposition to *always* combine iridectomy with extraction was made some time ago, as may be seen by referring to the following passage from my writings *(loc. cit.)* published nine years ago:—"Were any one to conceive the idea of making a pupil upwards a few weeks before extracting, as was in fact proposed to me by several, the only objection I should have to offer would be that in the infinitely greater number of cases the thing is unnecessary, and would be hardly compatible with the limited sojourn of the patients. On the other hand, such a procedure might be defensible on the ground of safety and as a prophy-

In the case of our patient, iridectomy is clearly indicated on the ground of probable suppuration of the flap.

It remains to decide whether iridectomy shall be done *at the same time* with the extraction, or shall *precede* it. The latter is, in my opinion, more judicious. Where the eye is unsteady its simultaneous performance is attended with certain practical difficulties or at least annoyances; also, with the disadvantage of a subsequent small hæmorrhage into the anterior chamber, which although in itself insignificant, may embarrass the third and fourth steps of a modified linear extraction, when iridectomy has been done as second act; moreover, while the recent formation of a coloboma certainly diminishes the tendency to a general iritis, the freshly cut edges of the coloboma do still incline to inflammation, resulting in union with the capsule, when irritated by the passage of the lens. These objections disappear when we find the coloboma ready made; the operation has then only its three usual steps, and is more quickly completed. I think that too short an *interval* should not be allowed to elapse between the iridectomy and the extraction. It is not advisable to do the operation within less than four, if possible six weeks of the other. Although the more evident signs of a tendency to irritability, consequent on iridectomy, may have disappeared, yet their traces may be observed in the redness of the parts met with after sleep, or in the minute infiltration in the neighboring portion of the cornea, revealed by oblique illumination; experience, moreover, shows that a too rapid succession of operations exerts a cumulative effect on circumstances disposing to inflammation, which, where the disposition to recovery is not a decided one, may give an impetus in the wrong direction. In short, if circumstances do not permit a longer interval than four weeks, I do the whole thing at one sitting, preferring this course, notwithstanding its disadvantages, to a double operation.

The poor patient whose case is in question is unable, owing to external circumstances, to lengthen her stay beyond what is absolutely necessary, still less to come back again. We have either, then, to do the iridectomy a week or ten days in advance, in contravention of the principle above established, or perform it simultaneously.

When, during the operation, unforeseen circumstances render necessary the performance of iridectomy, it forms of course the fourth step. Where the patient is restless, it is certainly attended with difficulty. Not infrequently too small a piece of iris is excised, the operator fearing to draw it too far forward, or dreading that an unexpected movement of the eye may give rise to dialysis or a loss of vitreous. Doing the operation, however, as the second step, immedi-

lactic." With people who have only one eye, I have invariably practised this procedure for several years, for although, where other things are favorable, the danger of suppuration or iritis is not imminent, yet it must be taken into account in every extraction, and the lessened probability of it which is brought about by an iridectomy seems to me to outweigh the objections already referred to, especially when we consider the disastrous consequence of a want of success.

ately after the completion of the corneal section, involves much less difficulty, and is of course to be advised in all cases where, as in the present instance, the performance of iridectomy had been previously determined on.

[To be continued.]

RHIGOLENE,*

A PETROLEUM NAPHTHA FOR PRODUCING ANÆSTHESIA BY FREEZING.

[Read before the Boston Society for Medical Improvement, April 9th, 1866,† and communicated for the Boston Medical and Surgical Journal.]

By Henry J. Bigelow, M.D., Professor of Surgery in the Massachusetts Medical College.

The above name is proposed as convenient to designate a petroleum naphtha boiling at 70° F., one of the most volatile liquids obtained by the distillation of petroleum, and which has been applied to the production of cold by evaporation. It is a hydrocarbon, wholly destitute of oxygen, and is the lightest of all known liquids, having a specific gravity of 0.625. It has been shown that petroleum, vaporized and carefully condensed at different temperatures, offers a regular series of products which present more material differences than that of their degree of volatility,‡ and that the present product is probably a combination of some of the known products of petroleum with those volatile and gaseous ones not yet fully examined, and to which this fluid owes its great volatility. A few of these combinations are already known in trade, as benzolene, kerosene, kerosolene, gasolene, &c., all of them naphthas, but varying with different manufacturers. I procured, in 1861, a quantity of kerosolene§ of four different densities, and found the lightest of them, the boiling point of which was about 90°, to be an efficient anæsthetic by inhalation.‖ When it was learned here that Mr. Richardson, of London, had produced a useful anæsthesia by freezing through the agency of ether vapor, reducing the temperature to 6° below zero, F., it occurred to me that a very

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* Rhigolene, from ῥιγος, *extreme cold*, to which is added the euphonious termination of most of the other petroleum naphthas.

† About three weeks after my first experiments with rhigolene, I first learned that Prof. Simpson, of Edinburgh, had lately employed "kerosolene" for this purpose.

‡ See Researches on the Volatile Hydrocarbons, with references to authorities, by C. M. Warren. American Journal of Science and Arts, July, September and November, 1865.

§ The kerosolene was furnished by Mr. Merrill, Superintendent of the Downer Kerosene Oil Co., South Boston.

‖ An account of these experiments may be found in this Journal, July 11, 1861. Reference is made to them in a paper "On the most Volatile Constituents of American Petroleum," by Edmund Ronalds, Ph.D., in the Journal of the Chemical Society, London, February, 1865. Mr. Ronalds there states that "the most volatile liquid obtained by collecting the first runnings from the stills employed in the process of refining petroleum has a specific gravity of 0.666." He had also received a specimen of "kerosolene" from Prof. Simpson, of Edinburgh, at 0.633. It will be observed that the Rhigolene has a specific gravity of 0.625.

volatile product of petroleum might be more sure to congeal the tissues, besides being far less expensive, than ether. Mr. Merrill having, at my request, manufactured a liquid of which the boiling point was 70° F., it proved that the mercury was easily depressed by this agent to 19° below zero, and that the skin could be with certainty frozen hard in five or ten seconds. A lower temperature might doubtless be produced, were it not for the ice which surrounds the bulb of the thermometer. This result may be approximately effected by the common and familiar "spray producer," the concentric tubes of Mr. Richardson not being absolutely necessary to congeal the tissues with the rhigolene, as in his experiments with common ether. I have for convenience used a glass phial, through the cork of which passes a metal tube for the fluid, the air-tube being outside, and bent at its extremity so as to meet the fluid-tube at right angles, at some distance from the neck of the bottle. Air is not admitted to the bottle, as in Mr. Richardson's apparatus, the vapor of the rhigolene generated by the warmth of the hand applied externally being sufficient to prevent a vacuum and to ensure its free delivery; 15° below zero is easily produced by this apparatus. The bottle, when not in use, should be kept tightly corked, a precaution by no means superfluous, as the liquid readily loses its more volatile parts by evaporation, leaving a denser and consequently less efficient residue. In this, and in several more expensive forms of apparatus in metal, both with and without the concentric tubes, I have found the sizes of 72 and 78 of Stubs's steel wire gauge to work well for the air and fluid orifices respectively; and it may be added that metal points reduced to sharp edges are preferable to glass, which, by its non-conducting properties, allows the orifices to become obstructed by frozen aqueous vapor.

Freezing by rhigolene is far more sure than by ether, as suggested by Mr. Richardson, inasmuch as common ether, boiling only at about 96° instead of 70°, often fails to produce an adequate degree of cold. The rhigolene is more convenient and more easily controlled than the freezing mixtures hitherto employed. Being quick in its action, inexpensive and comparatively odorless, it will supersede general or local anæsthesia by ether or chloroform for small operations and in private houses. The opening of felons and other abscesses, the removal of small tumors, small incisions, excisions and evulsions, and perhaps the extraction of teeth, may be thus effected with admirable ease and certainty; and for these purposes surgeons will use it, as also, perhaps, for the relief of neuralgia, chronic rheumatism, &c., and as a styptic, and for the destruction by freezing of erectile and other growths. But for large operations it is obviously less convenient than general anæsthesia, and will never supersede it. Applied to the skin, a first degree of congelation is evanescent; if protracted longer, it is followed by redness and desqua-

mation, which may be possibly averted by the local bleeding of an incision; but if continued or used on a large scale, the dangers of frost-bite and mortification must be imminent.*

It may be superfluous to add that both the liquid and the vapor of rhigolene are highly inflammable.

Reports of Medical Societies.

EXTRACTS FROM THE RECORDS OF THE BOSTON SOCIETY FOR MEDICAL IMPROVEMENT. BY CHARLES D. HOMANS, M.D., SECRETARY.

FEB. 12TH.—*Diphtheria.*—Dr. OLIVER reported the case. A child 12 years of age had been sick for ten days when first seen by him. There had been an exudation on one of the tonsils which had disappeared. Within the last three or four days a membranous exudation had made its appearance, so that when seen by Dr. Oliver there was a thick membrane on the right tonsil, extending to the soft palate and to the posterior pillar of the fauces, and considerable swelling of the neck of the same side. Three days subsequently, in the evening, the child began to be hoarse, and on the following morning the voice was nearly gone and the breathing much obstructed. Steam had already been resorted to, and in the hope of loosening the membrane the patient was made to inhale the spray of vinegar. After this considerable thick mucus was thrown off with some relief, but immediately a flapping sound was heard with each expiration. The face at once became very livid and suffocation seemed imminent. After endeavoring in vain to tear away the membrane with a sponge probang, trecheotomy was performed. Great relief to the symptoms followed, and the breathing remained unobstructed afterwards; death, however, took place thirty-six hours subsequently. The case occurred near the top of Beacon Hill, a locality about which cases of the disease had been rather frequent the last few weeks. Dr. Oliver remarked that this disease did not seem to prefer any particular season of the year, nor did it seem to be more frequent in low situations.

Dr. C. G. PAGE had seen lately in consultation (with Dr. Gray) a boy 5 years old. The case had not alarmed the parents until the night of visit, although the child had had "a cold" for three or four days. When first seen the dyspnœa was so great that death seemed imminent. The child was insensible; the mouth was opened with difficulty, and by the dim light at hand the tonsils were found very large, with a whitish grey deposit on the uvula; respiration stopped for thirty seconds after this attempt to examine the throat. Tracheotomy was immediately performed (at 4 A. M.), with at first great relief. Consciousness returned, and nourishment was taken, but on the afternoon of the same day the whole throat began to swell. There was some emphysema on the right side of the neck and chest. Death ensued twenty-eight hours after the operation.

The case was considered as croup when first seen, but the occurrence of diphtheria in the neighborhood, and the swelling of the throat

* The Rhigolene, from the Downer Kerosene Oil Co., may be obtained of Metcalf & Co., 39 Tremont St., Boston.

towards the termination of the case, threw some doubt on the correctness of the original diagnosis.

Dr. Page was struck in the eye while operating by a muco-purulent drop thrown violently out from the trachea, and had a mild attack of conjunctivitis a week afterwards.

Dr. WARE thought the diagnosis in young children sometimes very difficult. He had seen several cases of sore throat in the locality mentioned by Dr. Oliver, but in only one had there been a deposit on the tonsil, attended with great exhaustion. The constitutional disturbance generally was slight. He did not consider the simple fact of sudden death after apparent croup a proof of the existence of diphtheria. He had seen swelling of the neck with undoubted croup.

Dr. JACKSON said he had seen swelling of the glands and infiltration of the tissues of the neck in cases of croup before diphtheria was talked of.

FEB. 26th.—*Atrophy of the Bones of the Right Side of the Body, co-existing with Fracture of the Cranium on the Same Side.*—Dr. JEFFRIES WYMAN reported the case.

The bones here described were from a dissecting-room subject, the history of which is unknown. To the eye those of the two sides did not differ materially in size, though the ridges and tuberosities of the humerus were less strongly marked in the atrophied than in the normal bone. The differences in weight between the corresponding bones of the two sides are stated in the following table, in which the weight of each bone is given in grammes.

	Right.	*Left.*	*Difference.*	
Clavicle	18·9	22·1	3·2	
Scapula	59·2	82·7	23·5	
Humerus	114·8	182·0	67.2	
Radius	37·6	53.5	15·9	
Ulna	49·9	77·6	27·7	
Total weight of bones of arm	280·4	417·9	137·5	
Os innominatum	139·5	161·2	21·7	Left limb heaviest by 78·0.
Femur	343·9	391·2	47.3	
Tibia	231·5	225.3	6.2	Right leg heaviest by 9·9.
Fibula	53.9	50·2	3.7	
Total weight of bones of larger limb	768·8	827·9	59.1	

The ribs of the two sides were almost exactly of the same weight, differing only by a few millegrammes.

From the above table it appears that the long bones of the right arm were 137·5 grammes lighter than those of the left, and those of the right lower limb were 59·1 grammes lighter than those of the left. The right tibia and fibula together were, however, 9·9 grammes ***heavier*** than the same bones on the left side.

Co-existing with the condition of things just described, was an old fracture of the cranium, about an inch in diameter, situated near the middle of the frontal bone, but just to the right of the median line. The fragments of bone, which had been driven inwards so as to press upon the right fore lobe of the brain, were completely consolidated.

The fact that the atrophy was confined to the bones of one side of the body, renders it probable that it was the result of paralysis. If this

last be ascribed to the injury of the head, then there would be the unusual occurrence of the injury and paralysis existing on the same, instead of on opposite sides, as is commonly the case.

FEB. 26th.—*Cerebro-Spinal Meningitis; Latent until twelve hours before death.*—Dr. D. W. CHEEVER reported the case.

"Miss ——; 50 years of age; fair health. Complained Monday, Feb. 19th, of pain in foot, back and bowels, followed next day by nausea and vomiting, return of a pale discharge from vagina, menstruation having ceased one year previous. This discharge lasted three days.

"Wednesday evening, Feb. 21, 1866.—At 10 o'clock I saw her first; she had just gone to bed, having been up all day. Aspect a little flushed; pulse 100; nervous, irritable; complaining of bilious retching; no dejections for some days; flatulence, and pain in small of back. Ordered calomel and bi-carb soda.

"Thursday.—Two sufficient dejections; less retching and flatulence; aspect about the same; very nervous; restless; apparently exaggerates her troubles; much rheumatism in the family; complains of being unable to move freely, on account of her back, which is without feeling, she says, in lower part; somewhat painful; not tender; no paralysis of limbs or face. Eyes natural; no photophobia; hearing normal. Tongue moist, thin white coat; pulse 100. Ordered 10 grains Dover's powder; liniment to back, and hot drinks for diaphoresis.

"Friday, A.M.—Vomited Dover's powder, and continued to retch a good while, otherwise seems better. Skin moist. Tongue, thin, white coat. Pulse 80. A little desire for food. Says her back is better, and moves herself with more freedom. Ordered beef tea. 11, P.M., called to see her again. A very little delirious. Coherent on being roused. Complains that pain in back has shifted to nape of neck; restless, having had no good sleep for several nights. Tongue same; pulse 100. Skin soft and moist. Ordered one-fifth of a grain of morphia, and repeat, if necessary.

"Saturday, 9 A.M.—A bad night; restless; delirious; pain in nape of neck severe; jactitation; entire incoherency; vague apprehensions, seeking to avoid her attendants, creeping out of bed; moves limbs freely; no opisthotonos; pupils obey light; tongue brown and dry; pulse 130, weak and uneven; subsultus tendinum; urine passed voluntarily; abdomen not swollen; aspect of grave disease. Prognosis doubtful. Diagnosis, *Cerebro-spinal meningitis*, or *malignant typhoid*. 11 A.M., seen in consultation by a gentleman of large experience. To test the diagnosis from the history of the case, this was retailed to the consulting physician by a relative who had attended the patient throughout, and the case was closed with the events and treatment, up to last evening. The impression made on this gentleman was of a nervous case—probably, in a great measure, hysterical. On entering the chamber, he concurred with me in thinking she had but a few hours to live, and that the case was one of cerebro-spinal effusion. The patient was wholly insensible, lying on her right side, breathing heavily, but without stertor; covered with perspiration. Tongue brown and dry; mouth open. Pulse 160; very feeble and thready. Right pupil widely dilated and insensible to light; cornea looking as if breathed upon; left pupil natural, and responding sluggishly to light. Nothing abnormal in lungs, or heart; bowels soft

and not tympanitic; face a little drawn to right side; limbs not paralyzed. No opisthotonos; no petechiæ. In this condition she remained until she expired, in a convulsion, at 3 P. M.

No autopsy could be obtained.

The surroundings of the patient were of the best description. No other cases have been reported in that section of the city. And she herself was in average health the week previous, and up and about her chamber sixty hours before death. Can any other solution of the problem be offered so probable as cerebro-spinal meningitis?

Hospital Reports.

EXTRACTS FROM THE RECORDS OF THE ALBANY CITY HOSPITAL. BY G. TRESKATIS, M.D., LATE HOUSE PHYSICIAN AND SURGEON.

Hysterical Paralysis.—Dr. VANDERPOEL, attending physician.—Eliza McG., æt. 39, married, born in Ireland. Is of delicate, anæmic appearance; accustomed to hard work. Admitted March 10th, 1865. Has always enjoyed good health till last fall, when she felt some weakness in the limbs of the right side, which, however, was not sufficient to prevent her from attending to her usual work. About the middle of February pain supervened, which ceased after the administration of iron and quinia.

Symptoms on Admission.—Is unable to raise the right elbow-joint to a level with the shoulder. Complains of pain in the deltoid region. Cannot extend the fingers of the right hand after they are shut, especially the third and fifth finger. When walking, drags the right foot after her, sweeping the ground, as if a heavy weight were attached to it. Complains of pain in the temporal regions of the head. Is unable to put the tongue out straight. There is some impediment in speaking. No weakness or pain exists in the right side of the face. Secretions are normal. Pulse regular, but feeble. Appetite and sleep tolerable. Perfect rest was enjoined.

March 16.—Was ordered a tablespooful of cherry wine three times daily.

March 18.—Pain in the temporal regions of head has increased. Was ordered ferr. et. quin. citr., gr. ii., twice daily, two small blisters over the neck, and the application of slight electric shocks daily for ten or fifteen minutes. Her bowels not being moved, an injection was ordered.

March 22.—Has somewhat improved. Speaks better, can raise the right arm higher. A small blister was applied to the neck.

March 28.—Continues to improve. Has no difficulty in raising the right arm over the shoulder; extensor muscles of hand are stronger. Headache has disappeared entirely. Bowels remain constipated, have to be relieved every other day by an injection. A small hard swelling has appeared on the angle of the left jaw, which disappeared soon after external application of tincture of iodine.

April 10.—Electricity discontinued. Complains again of headache. A small blister was applied to the neck for several nights, which gave her great relief. Bowels have to be relieved by injections.

April 14.—Was accidentally frightened in the preceding night. Became very restless and nervous. Obstinate vomiting and sleeplessness followed, with severe headache and cold perspiration. Pulse slow and feeble. Fer. et quin. citr. was discontinued, and assafœtida, gr. x., ordered to be taken at bedtime. After these symptoms ceased, application of galvanism resumed and Huxham's tinct. ℥i. 3 times daily ordered.

May 4.—Has severe headache commencing at 5 P.M., which yielded, after two days, to the administration of quin. sulph., gr. v., at 4 P.M. Paralysis of the limbs of the right side has now disappeared entirely, only great weakness remains.

May 11.—Has recovered entirely; has no headache nor any difficulty in speaking. Is able to grasp things with her right hand, walks perfectly, and drags no longer the right foot behind. Bowels are relieved naturally. Was ordered quin. sulph., gr. i., with ammon. spir. aromat., gtt. xv., three times daily. Her pupils were dilated unusually for three days, but returned again to the natural size. Patient left the hospital May 20th, having recovered entirely. She is at present engaged as a servant, and enjoys good health.

Case II.—*Diagnosis obscure.* Dr. Vanderpoel, attending physician. Fred. A. P., aged 19, born in United States. Admitted to hospital April 7th, 1865.

History.—Was enticed by bounty brokers to the city, drugged with liquor and then abandoned.

Symptoms on admission.—Is unable to move the lower extremities; there is some tenderness over the whole body. Pulse and secretions are normal, eyes dull, tongue somewhat white, seemed to be in a state of apathy and dulness; when questioned gave no other answers but "yes, sir," or "no, sir." Was ordered quin. sulph., gr. i., three times daily.

Progress of the case.—Patient improved during the first week; could move his legs and walk to the closet. In the second week severe pain in back part of head and neck ensued; he vomited several times, and lost all appetite. Had rigors on the last three days of the week. Was ordered ferr. et. quin. citr., gr. ii., twice daily. In the third week pain in head and neck increased in severity, so as to cause him to cry out. Vomited several times in the latter part of the week.

May 1.—Had rigors in the evening.

May 2.—Severe pain in the back part of the head and neck. Pulse feeble.

May 3.—Vomited in the morning. Tenderness in right iliac region.

May 4.—Had rigors in the evening. Was ordered two-thirds of a grain of sulphate of quinine every four hours.

May 6.—Pain in neck and head is diminishing in severity. Pulse and appetite better. Tongue somewhat covered.

May 10.—Has improved continually since last entry. Appetite is good. Tongue clear, secretions regular. Is able to walk about.

Left the hospital perfectly restored, May 20, 1865.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON: THURSDAY, APRIL 19, 1866.

DR. SALISBURY'S DISCOVERY OF THE CAUSE OF INTERMITTENT FEVER.

It has come to be almost a matter of course in medical science that any one claiming to have made an important discovery is met at once by a counter-claim, by somebody who has anticipated him in every particular. There is either nothing in it, or it was known before. We ought not to be surprised, therefore, to find that the remarkable announcement of Dr. J. H. Salisbury, which we noticed some weeks since, that he had discovered the hitherto unknown cause of intermittent fever, has called forth a counter-claim. It is from the other side of the ocean, from France, that this claim comes. It is a discovery, it seems, announced so long since as August, 1864, and yet the medical world has been in profound ignorance of the fact until now! A few weeks since our editorial was made the subject of an article in the *Union Médicale*, and the discovery was laid before the readers of that journal as one of remarkable importance, and sure, if confirmed, to confer great distinction on the discoverer. The article has called out a communication from M. Lemaire, which the *Union* for March 27th publishes as follows :—

"It is with no less satisfaction that we enter a claim of priority concerning Professor Salisbury's discovery of the causative agent of intermittent fever, published in the last number but one. It ought never to be an ungrateful task to avow one's ignorance, to repair an error or omission ; it is much more pardonable than bad faith or partiality. We make the *amende* here all the more cheerfully that it has for its object the investigations of a laborious and distinguished confrère, Dr. Lemaire, the author of the Treatise on Phenic Acid, to whom we give place to clear up the history of this question.

"'In 1861, I demonstrated at the Museum of Paris to Professor Gratiolet, Doctors Senechal and Desmarets, assistants, that the gases which are disengaged from matters in an advanced stage of putrefaction always contain, in the vapor accompanying them, either spores or other germinal bodies of microphytes or microzoa. It is sufficient to condense this vapor by cold and to examine it under the microscope to demonstrate this.

"'I published the result of my first researches in the *Moniteur Scientifique* of Dr. Quesneville in 1862, in the number for the 15th of October. I made use of this discovery, at that time, in that journal, to maintain that the miasms which give origin to marsh fevers, &c., are living organisms. This paper was republished in the first edition of my book on Phenic Acid.

"'In 1864, leaving my laboratory, I went to Sologne with my friend Professor Gratiolet, to repeat my experiments on the miasms which are disengaged from the numerous swamps of that region. We chose those which were reputed the most unhealthy by the inhabitants.

"'We condensed, by means of cold, at the height of a metre [about

39½ inches] above the level of the marshes, the vapor which was given off; we examined it by our unassisted senses, by reagents and by the microscope. We observed that at the moment of condensation this liquid contained spherical, ovoid and fusiform spores, as well as a great number of pale cells of different dimensions. We found a considerable quantity of very small, semi-transparent bodies, of which I described the forms.

"'You will find the *résumé* of these researches, with others not less interesting on the same subject, in the *Comptes rendus* of the Academy of Sciences (sessions of 17th and 29th of August, 1864). I have published an extract from them in the second edition of my book on phenic acid, p. 188 and the following pages, and on page 355.

"'I have much to say with regard to the work which you have noticed, but I reserve my remarks for a full memoir on microphytes and microzoa, for which I have been collecting abundant materials for many years.

"'I have only wished to demonstrate to you that the honor of this discovery does not belong to Professor Salisbury. I hope that the information which I have given you is sufficient to clear up your mind on this point.

"'The experiments of Dr. Salisbury offer nothing new except the discovery of the microphytes in the expectoration of the sick. It remains now to be seen if other experiments confirm this assertion of the learned professor. I am, &c., J. LEMAIRE.'

"The priority of the discovery is thus incontestable. To France returns all the honor of it, for the vegetable origin of these febrigenous spores is but negatively settled. May their pathogenic interpretation be confirmed, and our learned compatriot thus acquire a new claim to the acknowledgments of all the friends of science."

So much for the claim of priority by Dr. Lemaire. In brief it may be stated to be, that in 1861 he demonstrated the presence of spores and other microscopic bodies in the emanations from putrescent matter; that in 1862 he suggested that fever-causing malaria is due to living organisms; that in 1864 he found microscopic forms of various kinds in the condensed vapor given off from an unhealthy marsh.

These observations are certainly extremely interesting and very important; so far as they go they confirm Dr. Salisbury's observations most fully and satisfactorily. But we contend that they do not transfer the honor of the discovery of the cause of intermittent fever from America to France, inasmuch as they stop short of demonstration. What Dr. Lemaire calls the only novelty in Dr. Salisbury's observations, contains, in our opinion, the great point of the discovery. Dr. Lemaire discovers certain bodies in gaseous emanations, and suggests that malarious diseases are caused by living organisms. Dr. Salisbury has done much more than this. He finds the bodies, eliminates certain distinct species, which he finds in different localities, examines the secretions of intermittent fever patients, detects the same bodies there, experiments with the poisonous algoid in a locality far removed from any spot where the disease had ever been known to exist, and actually *creates* in this way the disease in a number of persons! Perfecting his observations he is able to make out a number of species of these cryptogamous plants, and to discriminate those which give ori-

gin to remittent fever from those which produce intermittent. Surely he is entitled to some credit for all this. We would not deny to Dr. Lemaire any honor which his observations deserve. It is very plain that they had not produced much impression in Europe—if for no other reason, that a journalist in the same city announces Dr. Salisbury's discovery as a novelty of unusual importance, being at the time in complete ignorance of his compatriot's labors. At the same time we would remark that it is no novelty for intermittent fever to be *theoretically* attributed to an organic cause; but so far as we are at present informed, to Dr. Salisbury belongs the honor (if his observations are confirmed by others) of having *demonstrated*, and so made of practical importance, this wonderful discovery.

We would remark, in closing, that the original article in the *Union* was evidently based upon our editorial on the subject, although no reference to this JOURNAL appears. If the author had read the paper in full in the *American Journal of Medical Sciences* we are persuaded he would have seen very much more in it to admire and honor than it was possible for us to introduce into our pages.

The New Anæsthetic, Rhigolene.—Dr. Bigelow's communication in this week's JOURNAL is one of great interest and practical importance. The substance to which he gives the name rhigolene is probably the only known liquid which is capable of producing such powerful effects. Its great cheapness, its speedy evaporation without leaving behind any ill odor, and its very great convenience—enabling the operator to produce complete local anæsthesia without the loss of time, and the great inconvenience frequently accompanying anæsthesia by inhalation—make it of incalculable value to every surgeon. We are glad to learn that it will soon be supplied in quantity to the medical profession by the manufacturers mentioned in Dr. Bigelow's paper.

Chlorate of Potash in Ovarian Disease.—We have received from one of our most experienced practitioners the following note corcerning the article by Mr. Craig, with the above title, which was published in a late number of the JOURNAL.

"I saw this article when it first appeared, in the *Edinburgh Journal*, and although I could see no explanation for the action of the remedy, and had no faith in it, considered it a duty to try that which certainly is harmless. Extirpation, at the best, is a doubtful remedy in certain stages of the disease, and if all the *fatal* cases were published, we could judge better how early to make use of it. Certainly, medical treatment is worth using at the commencement.

"About the first of January I began Mr. Craig's treatment with a patient whose girth has, up to to-night, been reduced thirteen inches; a sequence, and perhaps a consequence.

"A second patient has lost an inch in girth within three weeks. So slight a change as this may be accidental, and of itself would not be worth mentioning.

"Two others under my observation have, as yet, small tumors, and I am waiting to positively form a diagnosis before beginning the same course.

"These four cases, at some future time, you shall have the notes of, whatever may be their termination. I hope the statement concerning the first may induce other gentlemen to try the chlorate." B.

Elephantiasis of the Integuments of the Penis in a Negro.—We have received from John Mountford, 272 Clark St., Chicago, a photograph of an enormous enlargement of the integuments of the penis by elephantiasis, said to weigh sixty pounds. The unfortunate bearer of this burden is being exhibited to the medical profession from city to city, and photographs may be obtained from Mr. Mountford, by mail, for thirty cents.

THE appointment of Dr. J. THEODORE HEARD to the position of Surgeon to Out-patients at the Massachusetts General Hospital, which should have been announced several weeks ago, will be received with pleasure.

VITAL STATISTICS OF BOSTON.

FOR THE WEEK ENDING SATURDAY, APRIL 14th, 1866.

DEATHS.

	Males.	*Females.*	*Total.*
Deaths during the week - - - - - - -	46	21	67
Ave. mortality of corresponding weeks for ten years, 1856—1866	38.4	35.9	74.3
Average corrected to increased population - - - - -	00	00	80.92
Death of persons above 90 - - - - - - - - -		0	0

ERRATUM.—*Messrs. Editors,*—In the communication on Relative Accommodation, in your last JOURNAL, page 215, line 16, the word "above" should be *about.* Will you please notice this, and oblige, G. H.

65 *Charles Street, April* 11, 1866.

COMMUNICATIONS RECEIVED.—Putrescent Sore Throat.—Is Asiatic Cholera Contagious?—Surgical Cases from the Records of the City Hospital. Seventh Paper.—Visit to Kirkbride Insane Asylum.—Report of a Case of Cerebral Disease. We must ask the indulgence of our friends for the unavoidable delay which has occurred in publishing their communications, arising from the accumulation of matter on our hands.

BOOKS AND PAMPHLETS RECEIVED.—Gonorrhœa and Syphilis. By Silas Durkee, M.D. Third Edition.—Recent Advances in Ophthalmic Science. By Henry W. Williams, M.D.—Principles of Surgery. By William Canniff.—Transactions of the American Medical Association. Vol. XVI.—Dictionnaire Annuel des Progrès des Sciences et Institutions Médicales. Deuxieme année. 1865. Par M. P. Garnier. Paris.—Intramural Interments in Populous Cities. By John H. Rauch, M.D. Chicago.—The Application of Sutures to Bone in recent Gun-shot Fractures. By John Birkett. London.

DIED,—In Charleston, Vt., of fatal jaundice, in his 57th year, Dr. Elijah Robinson. Dr. Robinson was a highly intelligent physician, and much esteemed and respected, not only among the medical faculty, but by those who had been the recipients of his professional care. His death will be deeply mourned by a large circle of friends and relatives, while the loss to his bereaved family will be irreparable. His disease came strictly under the head of Fatal Jaundice, as reported by Dr. Budd in his last edition on the Liver, which strongly assimilated, in almost every particular, *yellow fever*—a very uncommon development of disease in this northern section of country, so remote from crowded cities. D. A. R.

DEATHS IN BOSTON for the week ending Saturday noon, April 14th, 67. Males, 46—Females, 21. Accident, 2—anæmia, 1—apoplexy, 3—disease of the brain, 2—bronchitis, 1—cancer, 1—consumption, 16—convulsions, 3—croup, 2—diabetes, 1—diphtheria, 1—dropsy of the brain, 3—typhoid fever, 4—disease of the heart, 3—disease of the liver, 1—congestion of the lungs, 2—inflammation of the lungs, 3—marasmus, 1—old age, 1—paralysis, 1—premature birth, 3—disease of the stomach, 1—disease of the spine, 1—syphilis, 2—tumor, 1—unknown, 7.

Under 5 years of age, 21—between 5 and 20 years, 9—between 20 and 40 years, 17—between 40 and 60 years, 10—above 60 years, 10. Born in the United States, 43—Ireland, 17—other places, 7.